OUR BOOK SHELF.

The Elements of Geology. By Prof. W. H. Norton. Pp. x+461. (Boston, New York, and London: Ginn and Co., n.d.) Price 6*s*. 6*d*.

THIS is a further addition to the well written and well printed introductions to physical geography and geology which have been produced of late years for American schools. We do not quite agree with the author as to the novelty of the arrangement of his material, but it is certainly effective, and the questions attached to many of the illustrations are such as will draw out the reasoning powers of the pupil. Chemical and mineralogical considerations are kept in the background, and rocks are very broadly dealt with, as when syenite is defined (p. 274) as consisting of "feldspar and mica," and diorite as being "still less siliceous, composed of hornblende and feldspar—the latter mineral being of different variety from the feldspar of granite and syenite."

The book, however, hardly suffers from this, as explanations are brought in at the proper points, and a certain chemical knowledge seems to be presupposed. Zoological definitions are given from time to time in the stratigraphical portion, but we still think that the study of geology is held in too light estimation when it is thus regarded as elementary and independent, and not to be preceded by an outline of other branches of natural history. The geographical study of surface-features may, of course, be linked with geological considerations quite early in the curriculum of a school, and the portion of Prof. Norton's book that deals with the shaping of the earth's surface strikes us as especially admirable. It is confessedly and worthily based on the methods of Prof. W. M. Davis, who contributes a note of introduction.

The illustrations, moreover, are well selected throughout, and, where they have been borrowed, acknowledgment is made in most cases of the source. Mr. Welch's famous pot-hole in Glenariff, which was introduced to geologists in an English work in 1895, does duty here for the fourth or fifth time; but the numerous photographs of American scenery will prove of special interest to readers on this side of the Atlantic. The book is modern and very carefully thought out. On p. 246 volcanic phenomena are "extra illustrated" by the ruins of St. Pierre; on p. 306 the latest views are expressed on the gneisses of the "fundamental" Archæan complex; the Mesozoic reptiles receive attractive treatment on pp. 385–392; while on p. 446 a "restored" head of Pithecanthropus finds a place among relics of primitive man. This last instance errs, however, in showing much that is prophetic of future discoveries. Prof. Norton still translates *roches moutonnées* as "sheep backs," but the mention of this detail is only a tribute to his general accuracy.

G. A. J. C.

Letters from the Dead to the Dead. By Oliver Lector. Pp. 101. (London: Bernard Quaritch, 1905.) Price 6*s*. net.

THE fact that the letters attributed to Bacon, Shakespeare, Napier, Henry Briggs, and Guy Fawkes have dates attached to them about the years 1904 and 1905 should convince most readers that the book ought to be regarded in the light of a practical joke, in fact, a "take off" of the antique. As another example we may take the "Notes to Henry Briggs's letter," in particular the supposed proof (p. 75) that Napier's true base is the reciprocal of e and not e itself. As the difference depends entirely on whether log sin 45° has a plus or minus sign attached to it, it is interesting to speculate on how many readers will be deceived by what is, after all, a somewhat clever hoax.

Atlas colorié des Plantes et des Animaux des Côtes de France. By Dr. M. Langeron. Translated and adapted from P. Kuckuck's work. Pp. vii+67; with 24 coloured plates. (Paris: J. B. Baillière et Fils, 1906.) Price 7.50 francs.

THIS attractive publication is an adaptation, so far as the marine flora and fauna of France are concerned, of the well-known work of M. Kuckuck. It consists of three parts, dealing respectively with the phanerogams living in the neighbourhood of the sea, marine algæ, and marine fauna. The book is intended for the use of young students with a love of natural history, and should prove a useful companion during seaside holidays. The plates are instructive and well produced.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Osmotic Pressure.

AT Prof. Armstrong's request, published in NATURE of May 24 (p. 79), I willingly summarise the electrical evidence on which the theory of ionic dissociation seems to me to rest, though a full discussion of that evidence would, I fear, be too long for a letter in this place. Perhaps Prof. Armstrong will allow me to refer him to two papers in which I have written more fully what follows; one paper is in the *Philosophical Magazine* for February, 1903, and the other in the *Electro-Chemist* for July of the same year.

It will, I think, be admitted that we must accept the general view of electrolysis which we owe to Faraday and Kohlrausch, and imagine that opposite parts of the electrolyte move in opposite directions through the liquid. Such a view seems necessary to explain the appearance of the products of chemical change at the electrodes only, and may be verified by direct visual observation, as in the experimental measurements of the velocity with which those parts travel. It need involve no further assumption if, for convenience, we agree to call the moving parts ions. Controversy begins when we attempt to explain how and by what mechanism the ions move. The dissociation theory represents the ions as free from each other (though probably combined with the solvent) during that fraction of their lives in which they are concerned actively in conveying the current. Prof. Armstrong, on the other hand, rejects the idea of any kind of permanent or semi-permanent dissociation, and holds that the electrolyte exists in solution as combined molecules of, let us say, potassium chloride. On the latter theory the mobility of the ions must be secured by some mechanism which involves a series of interchanges between the opposite parts of the molecules. Grotthus pictured the molecules arranged as the links of a chain extending all the distance from one electrode to the other. This particular hypothesis would be exempt from the following criticism, but I suppose it would not be upheld by anyone at the present day. It conflicts too clearly with our knowledge of other phenomena. From our modern kinetic point of view, we should regard the molecules as in continual irregular motion, and suppose the ionic interchanges to occur at the instants when two molecules come within each others' sphere of influence.

Such, I imagine, is the alternative to the theory of ionic dissociation; let us trace its consequences. The greater the frequency with which molecular collisions occur, the more often can ionic interchanges take place, and the faster will the ions work their way through the liquid. Thus the ionic velocities will depend upon and increase with the frequency of collision—a frequency which, on the kinetic theory, varies as the square of the concentration. Thus, on the hypothesis we are considering, the velocities of the ions will increase with the concentration, and be

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proportional, approximately at any rate, to the square of the concentration. The conductivity, which is measured by the product of the number of ions and their mutual velocity, will then vary approximately as the cube of the concentration. But the facts are quite at variance with these conclusions. The velocities of the ions are found to be constant in dilute solutions, and to decrease slowly with increasing concentration when the solutions are stronger, while the conductivity of a dilute solution is proportional at most to the first power of the concentration.

We must, therefore, it seems to me, abandon the theory of ionic interchanges at the instants of molecular collision, and turn to the rival hypothesis. There are several pieces of positive evidence in favour of the view that the ions migrate independently of each other through the solution. Of these pieces of evidence I may mention three:—(1) the calculated velocity of an ion, as calculated from the conductivity and the migration constant, is independent of the nature of the other ion present when the solution is dilute—the velocity of chlorine is the same whether the other ion be potassium, or lithium, or sodium; (2) the difference of potential observed between two solutions of different concentrations can be calculated numerically on the assumption that the ions migrate independently of each other until the electrostatic forces of attraction prevent further separation; (3) it is possible to explain satisfactorily the curious relation between the valency of the metallic ion of a salt and its specific coagulative power on certain colloid solutions on the assumption that coagulation is due to the effect on the surface conditions produced by a certain minimum electric charge brought together by the chance conjunctions of isolated ions (*Phil. Mag.*, 1899).

The evidence sketched above has been obtained by observations on aqueous solutions. It remains a further question whether similar relations hold for solutions in other solvents and for such electrolytes as fused salts. In the latter case, at any rate, the conditions may be fundamentally different, and it seems necessary to wait for further investigation.

Such, it seems to me, is the present case for the theory of ionic dissociation when put in brief form and restricted to direct evidence. The indirect evidence, based on the success of similar ideas in coordinating and explaining the conduction of electricity through gases, however, must now be taken into account in any full discussion of the subject. I confess that to me the cumulative evidence seems overwhelming. But I hold no brief for the theory, as Prof. Armstrong seems to imagine, and if ever it ceases to be the best hypothesis in the field I shall willingly abandon it. Consistency always seems to me to be the meanest of the virtues, and in matters scientific it may become the most deadly of the vices.

I have endeavoured, as well as the exigencies of space admit, to comply with Prof. Armstrong's request. May I ask him in return to state his own view of the mechanism of electrolysis, to explain, in effect, what is the alternative he proposes to the theory of ionic dissociation?

I am glad to have given an opportunity for Mr. Norman Campbell to explain in public his interesting views on the subject of thermodynamics—views which we have discussed more than once in private without coming to an agreement, though, for my part, I have learnt much by Mr. Campbell's clear-sighted criticism. He will know what my answer to his letter must be, though I have no hope that he will be convinced thereby.

I do not wish to discuss the whole question of the theory of thermodynamics; I am not entitled to do so. But Mr. Campbell holds that, even if general thermodynamics be sound, its application to osmotics is not justified. Now it is here that we do not agree, and I should like to explain the grounds of my belief that not only is the application of thermodynamics to osmotics successful, but that the success is one of the strongest arguments we possess for the general validity of reversible thermodynamics as an ideal towards which practical arrangements may approximate.

Referring to the concordance between the theoretical value of the osmotic pressure of dilute solutions and that observed experimentally, Mr. Campbell says:—"It must be remembered that there is not perfect agreement. . . . The errors are larger than those involved in the direct measurement of the pressure and the other quantities involved; there is a systematic error. But this is due, say the thermodynamicists, to the imperfection of the membrane." I am not sure that I like to be called such a bad name, even by Mr. Campbell, but may I point out that Mr. Campbell has not put forward the whole of the evidence?

The direct measurement of osmotic pressure may not be a very accurate performance, though experience shows that as our membranes become more and more perfect the observed values more nearly approach that indicated by theory as the ideal case. But let us pass that by for a moment, and consider the relation between the osmotic pressure of a solution as defined theoretically and its freezing point. To do so we must use again thermodynamical reasoning, and introduce its necessary assumptions. But most of the assumptions will be the same as we made in deducing the theoretical value for the osmotic pressure, and they, at any rate, do not really add to the total number. Having obtained the well-known relation between osmotic pressure and freezing point, let us calculate numerically what the freezing point of the solution would be if the osmotic pressure possessed the theoretical value. With the most probable number for the latent heat of fusion, the molecular depression of the freezing point of water by the solution in it of a small quantity of any substance should be 1°·857 C. Mr. Griffith's experiments, made with all the refinements of platinum thermometry, gave for the molecular depression of solutions of cane-sugar (from a concentration of 0·0005 to 0·02 gram-molecules per litre) the number 1°·858. The limiting number for solutions of potassium chloride was 3°·720, half of which is 1°·860. Now these numbers agree with astonishing closeness; the opposite errors on Mr. Campbell's hypothesis must balance each other to within very little more than one part in a thousand. I cannot disprove his contention, but I think the balance of probability is against him, and, after all, any evidence which involves experiment must be merely a question of less or greater probability.

Now it seems to me that in this concordance we have a very strong piece of evidence in favour of the existence of an effectively reversible operation in the process of freezing—an operation much more nearly truly reversible than any it is possible to obtain with the rough and crude arrangements of even the best practical heat engines. Hence my present opinion is that, not only is the application of thermodynamics to osmotic phenomena in their theoretical aspect justified, but that it gives one of the most valuable confirmations we possess of the general validity of thermodynamic reasoning as a means of discovering an ideal limit towards which practical processes may tend.

In conclusion, may I point out the great interest of Mr. Campbell's argument that it may be "the swifter molecules which escape into the vapour and the slower which escape into the solid." The contention seems reasonable; and, though I feel that the strength of the case I have stated above is so great as to indicate that such a sifting of molecules cannot affect appreciably the thermodynamic result, it is clear that Mr. Campbell's point should be considered. Has Mr. Campbell really caught a Maxwellian demon? If so, all the world will crowd to study the specimen. But this letter is too long already, and I must leave the discussion to some one more learned than I am in this particular branch of demonology.

W. C. D. Whetham.

Trinity College, Cambridge, May 26.

A New Vesuvian Mineral.

Among the ejected masses that strew the flanks of Vesuvius many minerals of known species are to be recognised, and will be referred to in a publication now in preparation.

Perhaps the most interesting among a large number of blocks that I broke open after the eruption were two containing fine crystals of halite associated in spots with a canary-yellow mineral. This latter occurs in crystals up to several millimetres in diameter, and, so far as I have yet examined it, appears to be in rather flat rhombohedra. It is somewhat deliquescent, and therefore requires special

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